Comments on the ECOFRAM Terrestrial Draft Report

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A review of the ECOFRAM Terrestrial Draft Report (the Report) sections regarding terrestrial wildlife exposure was conducted with the following targeted goals:

1. Specific responses to the five questions submitted to ECOFRAM workshop panel members.

2. Identification of generic and specific comments regarding the methods proposed for conducting terrestrial vertebrate (specifically bird) exposure assessments.

Response to Five Questions

1. ***Is the draft report scientifically sound?***

In general, the models developed for terrestrial oral exposure appear to include the variables commonly considered to be important to assessment of exposure by that route. The theoretical mechanisms for exposure are well presented. In addition, many of the discussions of variables are accompanied by a limited discussion of the available relevant published data. However, there appears to be a general lack of empirical evidence to support the validity of the models *in toto*. This precludes a determination of whether the exposure models, as employed, yield hypotheses of risk that are supported by empirical data (i.e., they are scientifically valid in their outcome). The absence of case studies, involving chemical use combinations with multiple levels of exposure and effect assessment are a critical limitation of the Report. To their credit, this limitation is discussed by the framers of the Report. Developing case studies to determine the scientific validity of the models appears to be beyond the scope of the ECOFRAM charge and therefore remains a step that most likely will fall under actual implementation efforts.

2. ***Did the ECOFRAM Workgroup address the "Charge to the terrestrial and Aquatic Workgroups" identified in the background document, "Evaluating Ecological Risk; Developing FIFRA Probabilistic Tools and Processes"?***

This charge includes the following statement: "*Methods should be specific enough to allow different risk assessors supplied with the same information to estimate similar values of risk.*" The reviewer believes that the non-tiered approach defined by Chapter 6 of the document gives considerable latitude to the risk assessor with respect to the selection of levels of refinement for individual exposure parameters. While it is conceivable that sensitivity analysis of variable influence on outcome distributions will limit the selection of variables for refinement, there is little guidance in the Report that

would establish an appropriately rigid process that would engender consistency of results between individual risk assessment teams supplied with the same data. The report should be modified to better outline a rigid step-wise series of refinements to the exposure assesment. This guidance outline should succinctly describe the triggers for individual variable refinement, the additional data requirements and preferred collection methods necessary for refinement steps, and discuss the value added (e.g., increased parameter distribution information) and difficulties (e.g., extrapolation uncertainties for situation-specific data sets) associated with each refinement step.

Indirect effects as listed in the charge are discussed only cursorily in the Report. Although this meets the letter of the charge to "account" for indirect effects, the Report admits that the resources of ECOFRAM were largely limited to direct effects, and these were intimated by the charge to be of primary consideration.

The Report does not provide approaches to assess risk to terrestrial "communities and ecosystems" as discussed in the charge. However, it should be noted that it is the expectation of the reviewer that any such methods at this time would be highly uncertain and are not likely a useful expenditure of ECOFRAM resources. Furthermore, the charge to ECOFRAM explicitly recognized the time and resource limitations of the group and suggested that the initial effort consider individual and population levels of biological organization first.

3. ***What are the limitations for predicting risk using the approach described in the report?***

As is evident in the Report, there exist a variety of variables incorporated into modeling efforts for which the Agency has little or no suitable data to characterize the central tendencies or variability about these central measures. Assuming point estimates for these variables leaves much of the inherent variability left uncertain. Furthermore, assuming generic distributions for these input parameters, tells us very little of the actual central tendency and variability of the variable specific to the chemical/use under assessment, save for the expectation that they are somewhere encompassed by the generic. The overall limitation for predicting risk using the approach described in the report is the extent to which point estimates or generically assumed distributions are incorporated into modeling that is employed to predict reality. Overall uncertainty of model output should be closely considered. This is not the variability of output as it is impacted by input distributions, but the uncertainty associated with models based on very incomplete address of assumptions for specific parameters and the potential for unaccounted and unidentified parameters to affect targeted output under actual field conditions.

It is the reviewer's opinion that exposure assessments for the oral route should, at present, be limited to Levels 1 and 2 as depicted in Table 3.12-1. To extend the refinement of avian risk assessments, established on the basis of oral exposures alone, to more data intensive levels that additional modeling and collection of data for the attendant variables in such models would result in an overestimated level of certainty in the outcomes in the face of the unaccounted contributions of other sources of exposure.

4. ***What areas of the report need to be strengthened?***

See generic and specific comments in the section below.

5. ***At what point in the risk assessment process is the certainty level high enough to support the consideration of risk mitigation? What is the minimum level of technical information and scientific understanding that is necessary to evaluate whether risk mitigation would be necessary and/or effective?***

The consideration of mitigation efforts is a risk management decision. Consequently, the point in the risk assessment process where mitigation should be considered is dictated by the "threshold of acceptability" for the risk assessment that is determined by the risk manager. The risk manager should consider the economic impact of mitigation efforts prior to deciding upon a level of acceptable certainty for the risk assessment that would trigger mitigation consideration. If mitigation efforts are likely to be costly, consideration of the mitigation should be postponed until empirical data are generated for toxicity to multiple avian species, and exposure modeling is well founded in chemical-specific, biologically-specific and use-specific data so as to minimize costly consequences of extrapolation error.

The type of mitigation proposed should be a principal consideration when determining the level of technical information required for decision on implementation. If mitigations are based on modifying the application rate, then more generic levels of exposure assessment (usually highly dependent upon application rate) combined with effects assessment methods leading to estimates of the magnitude of effects (not just RQs) would be warranted and so toxicological data must be at the appropriate level to provide dose-response information. If mitigation methods involve establishment of buffer zones, spray drift modeling and field data specific to the ecological characteristics of areas adjacent to treated fields would be required. If methods involve the modification of the timing of application, risk assessments should be based on focal species so that the temporal and geographic characteristics of the focal species' use of treated environments can be assessed.

Generic Comments on Overall Approach

1. The reviewer is concerned with the Report assumption that the Level 1 exposure analysis represents a highly conservative screening assessment. Although the logic of such an assumption appears to be well presented in such examples as the chlorpyrifos case in Appendix C-10, the reviewer is aware of situations where default assumptions (presumed to be highly unlikely) regarding residues on dietary items are met or exceeded by measurements of composite sample residues. If composite residues (one could argue that these are representative of mean residues) are meeting or exceeding the "maximum" residues of Fletcher et al. 1994, one may rightly question the conservatism of the assumptions. Similarly, existing EFED screening methods, very similar to those presented in the Report have resulted in chemical/crop specific presumptions of risk that were at the

threshold of “acceptable”, but upon examination in the field were not rejected. One is compelled to ask the Type I and Type II error probabilities associated with the proposed screening method. Are there sufficient data or test cases to determine the error rate of the screening method? Should not this be determined before “acceptable” outcomes of screening-level assessments be viewed as a finishing point for the assessment process? Indeed, given the uncertainties surrounding reliance upon oral exposures alone as the dominant contributor to toxic risk, just what should be viewed as an acceptable Level 1 assessment outcome?

2. There are conflicting rationale for electing to expand a risk assessment to higher levels of refinement. In some areas of the document, the decision seems to be, rightly stated, to expand data collection and analysis to reduce uncertainty (establish better estimates of parameter central tendency and variation). However, other areas of the document, specifically page 6-15, suggest that enhancing the understanding of the variation and central tendency of a variable should be driven by the need to “reduce the risk below threshold”. The reviewer would like to stress that Agency decisions on higher tiers of risk assessment should be driven by the need to reduce uncertainty in any parameters that contribute significantly to overall assessment uncertainty and that this focus should not be limited to expectations that the outcome of such enhanced assessments will necessarily reduce risk assessment’s outcome

   It is not entirely clear who is making the decisions to expand on the analysis for a particular variable. Is this within the purview of the registrant (there are areas suggesting a cost benefit analysis appropriate to the industry) or is this within the Agency’s purview? The reviewer is concerned that decisions to expand the data collection and analysis specific to a particular variable, if left to the registrant, would lead to certain inevitable bias.

3. The overall process (Chapter 6) represents the “threshold of acceptability’ as being a broad band rather than a line, because “its position can vary from case to case and may never be defined precisely”. The reviewer is concerned that such a definition is going to preclude any consistency in comparisons between chemical/use combinations. Further, this definition is unaccompanied by any exacting guidance to risk managers is therefore likely to result in risk management decisions that could reflect individual manager technical sophistication, experience, philosophy, and personality to the extent that such decisions could be construed as being arbitrary between chemicals.

4. The reviewer agrees with the iterative approach presented in the Report for enhancing exposure assessments. However, it is apparent that the data requirements of each iteration are increasingly “place-based” such that data used to address uncertain variables (e.g., PT) are generated from studies of examples of specific agroenvironments. This link with the study area is not problematic when risk assessments and subsequent management decisions are targeted to a particular site (indeed many of the references used in the Report to develop approaches for exposure assessment are associated with applications of the risk assessment practice to hazardous waste sites). However, as these data become

increasingly dedicated to individual agroenvironments, the risk assessor must consider the uncertainty of extrapolating the information to broader geographic scales. This may not be a large concern for small scale specialty crops, but large scale crops (e.g., corn and rice) present difficulties associated with widely varying agroenvironments. An option to address this problem of extrapolation is to collect data form a variety of agroenvironments that are representative of the continuum of variable combinations and permutations. ECOFRAM should consider an expanded discussion of this problem and propose options for addressing the issue, perhaps going so far as to describe an approach for establishing a minimum number of sites deemed suitable to reduce extrapolation uncertainty.

5. The Report mentions testing for dependency between variables as a component of acceptable probabilistic risk assessment practice. The reviewer agrees with this requirement, with the hope that such measures will prevent unrealistic combinations of variable values. However, as discussed above, the generation of data to address uncertainties becomes increasingly linked to the study areas. Unfortunately, if a number of potentially interrelated variables are measured at separate study sites, the ability to effectively determine the potential for interdependence becomes limited. For example, residues on insects over time may be strongly dependent upon residues in vegetation or soils, but sampling for these variables on different sites of application my obscure this interdependence. ECOFRAM should devote some resources to the investigation and discussion of this potential problem.

Specific Comments Regarding Exposure Assessment Methods

1. The reviewer has concerns regarding the use of the avoidance factor (AV_{ijk}) in the overall food ingestion rate equations (page 3-10 and explanation on pages 3-32 to 3-36). The default for this factor is assumed to be unity, unless data are available to support selection of a value less than unity. The reviewer believes that avoidance factors must be carefully used in a manner that is protective of avian life stages that may not exhibit avoidance behavior. Specifically, altricial nestlings may not exhibit such behavior. Unless avoidance behavior can be demonstrated to be applicable to parental selection of food items for offspring, an assumption that avoidance is protective of nestlings would be erroneous.

   Further, the reviewer is concerned with the unidirectional nature of this variable (i.e., the value is defaulted to zero and provisions are made for positive values between zero and 1). Is it possible that intoxication of invertebrate or vertebrate food items may make these organisms more susceptible to predation? Would this have implications for increased attractiveness of residue-impacted dietary items such that the avoidance factor would be less than zero? Has ECOFRAM considered the possibility that certain pesticides, tank mixes, or formulations might be attractive to wildlife? For example, bait traps used in conjunction with pesticides may result in high concentrations of exposed target pests that would be attractive to wildlife. Are there empirical data in the literature to support such a concern? Would such attractiveness factors be more readily incorporated into this variable or in the variable PT? If PT is a more appropriate variable for inclusion of this affect, should not experimental studies for establishing PT include application of the pesticide?

Finally, the reviewer is concerned about the interspecific nature of the avoidance factor. Does ECOFRAM suggest that avoidance factors be developed for specific focal species? This is likely to be a prohibitively expensive undertaking. How can empirically determined avoidance factors be extrapolated between species, given the likelihood that different feeding guilds may actually operate on different sensory ques for selection of food? Are there sufficient data at present to propose extrapolations? Should this be a new data requirement for registration and re-registration of chemicals?

2. The reviewer is concerned with the simplified exposure model that partitions avian habitats into treated and untreated areas suggested for the Level 2 assessment (Table 3.12-1). As the Report indicates, there is seldom a clear demarcation between those areas receiving pesticide application and those areas not impacted by the chemical. The simplistic model may be most applicable for very targeted application technologies (e.g. between disc in furrow applications), where spray drift may be minimal. However, the ECOFRAM report should caution the reader that under spray applications where drift is an issue, the simplistic variable expressing the proportion of diet originating in the treated area should be based on expansion of the treated area to reasonably include areas subjected to some minimal level of drift. Naturally, the drift area included in the treatment would vary with offsite predictions of drift (as influenced by application characteristics important to drift such as droplet size and meteorologic conditions). Table 3.12-1 should be modified for the variables PT and C to include reference to drift modeling as input into these variables at Level 2 of refinement.

3. The Report indicates (page 3-26) that a screening-level total food intake rate (TFIR) could be estimated with existing information on actual intake rates (presumably this is available data from the literature on empirically measured intakes). The reviewer believes that ECOFRAM should more strongly urge caution with this approach, as measured intake rates can be highly circumstantial. The Report should discus the need for risk assessors to carefully consider these circumstances in light of the expected environmental conditions expected for the agroenvironment under consideration. For example, measured data on American robin intake rate estimates reported in the USEPA Wildlife Exposure Factors Handbook (USEPA 1993) include a high estimate of food intake rate that is entirely based on the consumption of berries. An assumption of exposure to pesticide residues from a diet consisting of mixed items (seeds, fruit, and invertebrates) as might be expected in natural areas surrounding agricultural fields during much of the growing season, would result in inaccurate estimates of pesticide exposure if this maximal intake rate was assumed.

   The Report also suggests that the equations of Nagy (1987) could be used as an alternative. If this suggestion refers to the allometric equations of Nagy (1997) specific to selected types of birds, then ECOFRAM should more obviously suggest that the assumed dietary characteristics should also be incorporated because application of these generalized food intake models for diets departing from the Nagy assumptions could result in less than conservative pesticide exposure estimates. The Report suggests that a 2-3 fold factor (3-fold in Table 3.12-1) for the daily TFIR would be a conservative assumption. However,

given that the upper 95% CI of the mean Nagy predictions (for passerines) are already about 1.5 times higher than the mean and that examples of within-study variation of mean estimates of bird ingestion can be very substantial (e.g., see American robin ingestion rates range from USEPA 1993), ECOFRAM may consider expanding the statement "birds and mammals may reasonably increase TFIR 2 to 3 fold after short bouts of starvation in poor weather" (page 3-23) with some appropriate discussion of the literature used to establish these factors for starvation and poor weather. Further, additional discussion of the Kirkwood (1983) reference appears warranted. The reviewer is also not sure of the relevance of trained pigeon feeding behavior (Pascual et al. in press) to wild bird variability in ingestion rate, perhaps further discussion of the study would be helpful.

Finally, the Report is conspicuous in the absence of accounting for the dietary requirement of nestlings and precocial young at the screening level, and an obvious question arises as to whether exposure estimates based on food intake rates for adults will adequately protect young. Although the report does indicate that life stage may be an important factor in determining food intake rate, the report does not provide the reader with guidance on how applicable the 2-3 fold adjustment factor is to early life stages. The reviewer recommends that ECOFRAM consider additional research into the available literature (perhaps on the differential energetics of nestlings and precocial young as compared to adults) before the screening methodology proposed can be determined to be adequately protective.

4. The Report includes a series of refinement levels for determining the values for C, the residues in a given food item. These levels of refinement for C are presented in Table 3.12-1 and progress from conservative assumptions using existing empirical relationships (e.g., Fletcher et al. 1994), to hypothetical distributions (based on confidence limits or the raw data) from available literature, to models (under development) to an ultimate of collection of actual measured residues. This progression to an ultimate level involving residue data collection seems to put a great deal of emphasis on models currently under development. We do not now have, nor will we in the future have, validation of these models without collection of the data from the highest levels of refinement as now proposed. One may then question whether theoretical model predictions of residues, that result in risks below some acceptable threshold, will ever receive adequate scrutiny through comparison with field generated data.

Is ECOFRAM aware of extant data involving pesticidal compounds that can currently be compared to models for predicting vegetation concentrations (e.g., equation 3.10-2 and those described in appendix C-4)? These complex models include numerous variables that appear to be data-hungry and the origin of appropriate data to address the variables is unclear at the present time. Are many of the variables assumed to be point estimates at the present time? Further the high degree to which the models are nested with additional models describing specific variables suggests considerable uncertainty regarding the overall effects of compounded error.

The reviewer would propose, in the absence of robust data sets to validate many of the

models designed to predict C for different compartments, that theoretical models for concentration not be included in the levels of refinement until there are sufficient empirical data to validate them. The reviewer proposes the establishment of expert panels to review these models under development and suggest a suitable course of empirical data collection for such validation. However, ECOFRAM should be commended for its presentation of the models as they remain an excellent way to identify parameters that, by expert judgement, may be considered to be important influences upon residues in food items. The variables incorporated in a model give guidance to monitoring efforts for validating output of the model.

5. The Report states (page 3-58) that dermal and inhalation doses cannot be combined with the oral ingestion dose to estimate a total dose. The pharmacological and toxicological reasoning for this is compelling, in general terms. However, one is left with the problem of how exactly the exposures and consequent toxicological risks from these varied routes are to be evaluated together. The risk assessor cannot quantitatively evaluate the risks of dermal and inhalation exposures without significant knowledge of the pharmacokinetics and toxicological characteristics and there appear to be no recommendations in subsequent chapters for conducting such evaluations. Is the risk assessor expected to finely hone the oral exposure risks alone, and declare other routes uncertain or (worse) unimportant?

   Under an appropriately conducted initial screening method it may be appropriate to suggest that the conservatism of the method could overwhelm contributions to overall risk by other routes of exposure. However, many of these conservatisms are stripped away as the assessment is refined. How confident can risk managers be with the results of such fine tuning in the face of unquantified risks from these other routes? Even the example of chlorpyrifos in apple orchards presented in the Report does not discuss the confounding implications of these other routes of exposure on the overall uncertainty of the assessment. More work is needed here to guide the assessor in accounting for routes of exposure outside the oral. Should the refinement of oral exposure be limited to the maximal levels of refinement for the analysis of risks from other routes of exposure?

6. The reviewer has concerns regarding the establishment of values for the variable PT (the proportion of diet originating from a treated area) in levels of assessment greater than Level 2. Regardless of the methodology employed in assessing the PT factor in the field, the reviewer believes that the data need to be generated under conditions that would warrant actual pesticide treatment. For example, it is reasonable to expect that insectivorous birds would optimize feeding time in environments with the maximal opportunity for success of predation. Therefore, one could expect that empirical assessment of the use of agricultural fields as a source of insectivorous diet would be dependant on actual insect pest load in the fields. If a chemical can be expected to be applied during periods of high pest load, then empirical measurement of agricultural fields as a dietary source could be underestimated by studies conducted during periods of low pest load. ECOFRAM should caution the reader that empirically-determined distributions of field use by wildlife as a source of diet should account (at least for short and medium term exposures) for high pest loads that would trigger application.

7. The reviewer believes the granular exposure models presented in the Report are an interesting approach to account for oral exposure. However, the discussion of the limitation of the exposure model to only direct ingestion of granules should be emphasized. There are apparently multiple avenues of exposure to granularly-applied chemical in addition to selection of granules for direct ingestion including, dissolution of material in standing water, adhesion of material to other food items, and dermal transfer of pesticide to granules to feet and body during foraging or dust-bathing activities. Although the reviewer could identify many of the sources of field-collected data that demonstrated the validity of selected parameters in the model as being important to exposure to granules via the direct oral ingestion route, the reviewer could not determine whether the granular exposure model as a whole has been validated in the field for a variety of species, fields, and granule types. (Perhaps this discussion was missed by the reviewer?)